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Working Paper Series

Do Vouchers Lead to Sorting under Random Private School Selection? Evidence from the Milwaukee Voucher Program

WORKING PAPER #09-02
March 2009

Rajashri Chakrabarti
Federal Reserve Bank of New York

Acknowledgements

I am grateful to Steve Coate, Ron Ehrenberg and Miguel Urquiola for helpful comments, suggestions and encouragement. I thank Brian Jacob, George Jakubson, Dean Lillard, Paul Peterson and seminar participants at the Econometric Society Meetings, American Education Finance Association conference, Southern Economic Association Conference for helpful discussions, and the Program on Education Policy and Governance at Harvard University for postdoctoral support. The views expressed in this paper are those of the author and do not necessarily reflect the position of the Federal Reserve Bank of New York or the Federal Reserve System. All errors are my own.

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Abstract

This paper analyzes the impact of voucher design on student sorting, and more specifically investigates whether there are feasible ways of designing vouchers that can reduce or eliminate student sorting. It studies these questions in the context of the first five years of the Milwaukee voucher program. Much of the existing literature investigates the question of sorting where private schools can screen students. However, the publicly funded U.S. voucher programs require private schools to accept all students unless oversubscribed and to pick students randomly if oversubscribed. This paper focuses on two crucial features of the Milwaukee voucher program—random private school selection and the absence of topping up of vouchers. In the context of a theoretical model, it argues that random private school selection alone cannot prevent student sorting. However, random private school selection coupled with the absence of topping up can preclude sorting by income, although there is still sorting by ability. Sorting by ability is not caused here by private school selection, but rather by parental self selection. Using a logit model and student level data from the Milwaukee voucher program for 1990-94, it then establishes that random selection has indeed taken place so that it provides an appropriate setting to test the corresponding theoretical predictions in the data. Next, using several alternative logit specifications, it demonstrates that these predictions are validated empirically. These findings have important policy implications.

1 Introduction

School choice and especially vouchers are among the most hotly debated instruments of public school reform. Several issues relating to vouchers have received widespread attention in the media as well as popular debate. One of the more important among these is the question of who takes advantage of the choice opportunity. Do vouchers lead to sorting or cream-skimming, that is, a flight of high income and more committed public school students and parents to the private sector? Or do they facilitate the movement of lower income and less able students to the private sector?

This paper analyzes the impact of voucher design on student sorting, and more specifically investigates whether there are feasible and realistic ways of designing vouchers that can eliminate sorting. Can the absence of private school screening obviate student sorting or do vouchers lead to sorting even under random private school selection? Much of the existing literature investigates the question of sorting in a framework where private schools can screen students on the basis of their observed characteristics. But the publicly funded voucher programs in the U.S. (Milwaukee, Cleveland, Florida and Washington DC) are characterized by an absence of private school selection, they typically require private schools to pick students randomly. The ability of private schools to choose their customers undoubtedly acts as a strong force in favor of cream-skimming. Consequently the question arises as to whether vouchers lead to cream-skimming when the private school selection factor is absent and private schools are required to accept students randomly. Another important feature of some of the publicly funded voucher programs in the U.S. (Milwaukee and Florida) is the absence of topping up of vouchers. Can the absence of topping up and the acceptance of vouchers as full payment of tuition avert sorting?

The paper analyzes these questions in the context of the first five years of the Milwaukee voucher program. Implemented in 1990, the Milwaukee Parental Choice Program (MPCP) made all Milwaukee Public Schools (MPS) students with family income at or below 175% of the poverty line eligible for vouchers to attend private schools. The program requires the voucher amount to be taken as full

payment of tuition by the private schools (that is, vouchers are not allowed to be topped up) and the latter are not permitted to discriminate between students. Specifically, the private schools have to accept all students unless oversubscribed and have to pick students randomly if they are oversubscribed. Even the student application form for the MPCP does not ask any question relating to the race, sex, parents' education, past scores of the student, other prior records (example, truancy, violence) etc. The questions asked are specifically geared only to ascertain whether the student is eligible for the program (that is, satisfies the income cutoff).

It should be noted here that this paper focuses on the first five years (1990-94)¹ of the Milwaukee voucher program. Following a Wisconsin Supreme Court ruling, the Milwaukee program underwent a major shift in 1998, when the religious schools were allowed to participate for the first time in the program. This led to a major expansion of the program, both in terms of the number of choice schools and in terms of the number of choice students. The majority of the private schools taking part in the program after this shift were religious schools, while the pre-1998 choice schools were all secular. Due to lack of availability of adequate student level data, this paper focuses on the smaller Milwaukee voucher program and the empirical findings pertain to that part of the program. The theoretical predictions of the paper should continue to hold in the second part of the program also as the key features that drive the results continue to remain the same, but there is no way to test their validity in the latter part of the program.

The paper develops its argument in the context of a theoretical framework where utility maximizing households care about two goods, school quality and a numeraire good. School quality available to the households can be of two types—public or private. The households are characterized by an income-ability² tuple. The purpose of the model is to analyze the implications of the two crucial features of the program—random private school selection and acceptance of vouchers as full payment of tuition—on

¹ For the remainder of the paper, I will refer to school years by the calendar year of the fall semester.

² Ability is taken here as a broad measure that captures the ability of the child, and the motivation and commitment of the parents and the child.

student sorting. In an equilibrium framework, the model predicts that random private school selection alone cannot avert sorting. However, random private school selection coupled with the absence of topping up (that is, a Milwaukee-type program) can preclude sorting by income, although there is still sorting by ability.

Sorting by ability is not caused here by the supply side factor of private school selection. Rather, it is induced by the demand side factor of parental self-selection. Vouchers undoubtedly enhance the choice opportunities of eligible students, however movement to private schools is associated with relocation costs such as acclimatization and time costs.³ Given income, only households that are more committed towards their child's education find it worthwhile to incur this cost and switch their child to a private school. This leads to sorting by ability. The driving force behind any sorting by income is that at a given ability level, households with different incomes are affected differentially by the private school alternative—only the high income households can afford private school tuition or to top up vouchers, while the low income ones prefer to forego such payment and stay back in public school. The absence of topping up of vouchers in the Milwaukee program avoids the differential effect on households in terms of income and hence prevents sorting by income.⁴

Using individual level demographic and survey data from Milwaukee for 1990-94, the paper next proceeds to test the relevant theoretical predictions. Implementing a logistic estimation strategy, it first establishes that private schools have indeed picked choice students randomly—there is no statistically significant difference between the successful and unsuccessful applicants on the basis of a variety of demographic and socio-economic characteristics. This sets the stage for testing the theoretical predictions. Using several alternative logistic specifications, I show that the theoretical predictions are

³ Switching schools exposes students to new teachers and curriculum, detaches them from their friends and social circles, interferes with extra-curricular activities, requires parents to make schedule changes and adjustments, requires parents to incur time costs to find a suitable private school and neighborhood etc.

⁴ However, note that as far as there are monetary costs of relocation, such as costs of buying new uniforms, books etc., there will still be some sorting by income. However, this cost is likely to be much less than the private school tuition or the payment over and above vouchers required by programs that allow topping up. Note that, under the MPCP, transportation costs of students switching to private schools with vouchers are borne by the MPS.

validated empirically. The findings are reasonably robust in that they survive several robustness and sensitivity checks.

The findings of the paper have important policy implications. Both the theoretical and empirical analysis of the paper strongly suggest that careful design can virtually eliminate sorting by income. Specifically, random private school selection coupled with acceptance of the voucher amount as full payment of private school tuition can preclude sorting by income. This design, however, cannot avoid sorting by ability as it is driven by the differences in interest and motivation of the parents towards their child's education.

The last decade has seen several theoretical and empirical studies that look at the issue of student sorting. Epple and Romano (1998) look at the effect of vouchers on the choice between public and private schools when private schools can screen students by means of tuition discounts. They show that even a simple public-private system (without vouchers) leads to stratification by income and ability, while vouchers exacerbate stratification. Using NELS (National Educational Longitudinal Survey) data, Epple, Figlio and Romano (2004) find considerable support in favor of this theoretical prediction. In the context of a computational model, Epple and Romano (2002) examine how alternative voucher designs can affect stratification and technical efficiency. In the presence of private school selection, they show that type-dependent vouchers (conditioned on student ability) that require schools to accept vouchers as tuition can prevent stratification.

Hsieh and Urquiola (2003) focus on the nationwide voucher program in Chile that was introduced in 1981. Using differences across around 300 municipalities, they show that wealthier families and families with higher education are much more likely to avail of vouchers. In the context of a national, means-tested school voucher program, Campbell, West and Peterson (2005) show that the chances of both application and actual voucher take-up increases with mother's education and decreases with family income, although the effects for voucher take-up are not statistically significant at conventional levels. They argue that the pattern for income may be a reflection of the program feature that vouchers awarded

to higher income families were smaller. They also find that religious motivations play an important role in choice decisions. On the one hand, Catholics and Evangelical Protestants were more likely to apply and take up vouchers, on the other, frequent church attendance had a significant impact on both stages of voucher usage. Peterson, Howell and Greene (1999) and Metcalf (2003) find that voucher students in Cleveland have higher mother's education and lower income than public school students. The latter pattern, both studies argue, is most likely due to the program rule that required vouchers to be first given to low income families. Howell et. al. (2002) and Howell (2004) find that higher income families are more likely to take a voucher in New York City, while Howell et. al. (2002) finds that low income families are more likely to do so in Dayton, with no difference observed in Washington, D.C. In a survey of previous literature, Levin (1998) finds that choosers are more advantaged both educationally and economically than non-choosers. Hoxby (2003) finds no evidence of cream-skimming in the context of charter school competition.

Most of the above studies explore the question of sorting where private schools can choose their customers. It may be noted, though, that some of them analyze the characteristics of the choice *applicants*,—this to some extent will yield the effects of random private school selection. However, anticipation of private school discrimination might affect the decision to apply and the applicant population in programs with private school selection may not be the same as that under random private school selection. This paper is interested in analyzing the issue of sorting where the supply side private school discrimination factor is completely absent.

The studies most closely related to this paper are Witte and Thorn (1996) and Witte (2000). They study the characteristics of students and families in the MPCP and the Chapter 220 program (an interdistrict program in Wisconsin aimed at racial balance). They find that the MPCP applicants had lower income, lower prior math scores, higher mother's education and higher parental involvement than their counterparts in the MPS.⁵ The chapter 220 participants had a higher income, higher mother's

⁵ The odds ratios for income, prior math scores, mother's education and public participation were 0.968, 0.982, 1.289, 1.082 respectively and the corresponding p-values were 0.007, 0.010, 0.002, 0.001. Thus the odds ratios for income and

education and higher prior scores than the choice applicants and their counterparts in the MPS. Witte and Thorn argue that parental self-selection and program constraint (income constraint that only the low income households could apply) were the key causal mechanisms in MPCP.

This study differs from Witte (2000) and Witte and Thorn (1996) in several important ways. First, the focus of this paper is different. It is interested in investigating the effect of random private school selection and the absence of topping up on sorting. For this purpose, unlike the above two studies, it first examines the implications of these factors in a theoretical framework and then tests the predictions empirically. Second, this study does a more careful empirical analysis in that the regressions allow the different measures of parental involvement to have different effects and controls for school, grade and year indicator variables. Inclusion of school indicator variables allow me to compare applicants and eligible non-applicants within schools. This rules out any role of school specific factors and ensures that the effect of parental actions is not confounded with that of school specific actions (example, enthusiastic administrators encouraging movement, or vice-versa). Moreover, the data on prior test scores pertain to different years, schools and grades, so that using the corresponding dummies rules out any contamination of the effects due to these factors. Finally, an important difference with Witte (2000) and Witte and Thorn (1996) as well as other studies mentioned above⁶ is that this study combines a theoretical framework and an empirical counterpart, the theoretical part designed to analyze the effects of the two crucial features of the Milwaukee program and the empirical part designed to test the theoretical predictions.

2 The Model

The model consists of a single group of agents—the households. Households are characterized by an income-ability tuple (y, α) , where $y \in [0, 1]$ and $\alpha \in [0, 1]$; y and α are assumed to be independently and

prior math scores were very close to one and only marginally significant.

⁶ Exceptions are Epple and Romano (1998) and Epple, Figlio and Romano (2004)—the latter study empirically tests the predictions obtained in the former.

uniformly distributed. The “ability” of the household is used here as a broad term that includes the ability of the child, seriousness and motivation of the parents, parents’ education levels, parents’ desire for child’s education etc. A household obtains utility (U) from the consumption of the numeraire good (x), school quality (θ) and its ability (α). The household utility function is assumed to be continuous and twice differentiable and is given by $U(x, \theta, \alpha) = h(x) + \alpha u(\theta)$. The functions h and u are increasing and strictly concave in x and θ respectively. It follows that households with higher ability, that is, those that are more motivated and committed, have a higher preference (marginal valuation) for school quality, $U_{\theta\alpha} > 0$.⁷ (This is a common assumption in the existing literature.)

School qualities available to a household are public school quality and a continuum of (exogenously given) private school qualities. The public school is free and offers exogenously given quality q to all households that choose to attend it. There are a continuum of private schools providing a continuum of quality. Each private school is modeled as “passive”. This is in keeping with the feature of the Milwaukee voucher experiment, by which private schools are not allowed to discriminate between students. They have to accept all students unless oversubscribed and have to accept students randomly when oversubscribed.⁸ Households pay a tuition $T = t \cdot Q$ ($t > 0$) to attend a private school of quality Q .⁹ A household incurs switching or relocation costs if it decides to switch from the public school to a private school. There are two kinds of relocation costs: (i) non-monetary or utility costs of relocation, c_1 (example, acclimatization and time costs),(ii) monetary costs of relocation, c_2 (example, costs of new uniforms, books etc.) The paper considers two alternative scenarios: (i) a simple public-private system, where the voucher amount is zero and (ii) a voucher system where vouchers take an exogenously given value v .

⁷ The assumption $U_{\alpha\alpha} = 0$ is made for simplicity. All results go through under $U_{\alpha\alpha} < 0$.

⁸ Of course, in the voucher experiment, they can choose whether or not to enter. I abstract from that here for simplicity.

⁹ Note that private school quality always exceeds public school quality. Otherwise, no household would pay to attend a private school.

3 Characterization of Equilibria

This subsection analyzes the household behavior under the two systems in a common framework. Under each of the two systems, households observe whether vouchers are imposed and make their utility maximizing decisions, given parameters q , t , c_1 and c_2 . Each household can either choose to go to the public or to a private school. In the former case, it gets utility $h(y) + \alpha u(q(e, b))$. In the latter case it gets utility $h(y + v - t \cdot Q^* - c_2) - c_1 + \alpha u(Q^*)$, where Q^* is the optimal private school quality choice of household (y, α) given v , t , c_1 and c_2 . The parameter v takes on a value of zero in the absence of vouchers. For the sake of generalization, I first assume that vouchers have to be topped up, that is $v < tQ^*$. A household (y, α) chooses private school iff $h(y + v - t \cdot Q^* - c_2) - c_1 + \alpha u(Q^*) > h(y) + \alpha u(q(e, b))$. Define $D = [h(y + v - t \cdot Q^* - c_2) - c_1 + \alpha u(Q^*)] - [h(y) + \alpha u(q(e, b))]$. Note that $\frac{\delta D}{\delta y} > 0$ and $\frac{\delta D}{\delta \alpha} > 0$ even when $v = 0$, so there is stratification by income and ability before the imposition of vouchers.

For each y and given t, v, q, c_1, c_2 , there exists a unique household $0 < \hat{\alpha} < 1$ such that all households with lower ability choose the public school and those with higher ability choose a private school.¹⁰ This $\hat{\alpha}$ is the unique solution to $[h(y + v - t \cdot Q^* - c_2) - c_1 + \alpha u(Q^*)] - [h(y) + \alpha u(q(e, b^e))] = 0$, where Q^* is the optimal private school quality choice of the household $(y, \hat{\alpha}(y))$.¹¹ Since the indirect utility function is continuously differentiable and $D_\alpha > 0$, by the implicit function theorem, $\hat{\alpha} = \hat{\alpha}(y; v, q, t, c_1, c_2)$ is a continuously differentiable function. Using the implicit function theorem it is straightforward to check that for each income level, the cutoff ability level $\hat{\alpha}$ is decreasing in v and increasing in q, t, c_1 and c_2 . Given all other parameters, the cutoff ability level varies inversely with y . This is because both higher income and higher ability increase preference towards private schooling.

Similarly, for each α and given t, v, q, c_1, c_2 , there exists a unique household \hat{y} such that all households with lower income choose public school and those with higher income choose private school. Again using

¹⁰ I assume that there are always some households in the public and some households in the private sector. This assumption is made for simplicity. All results go through as long as there is at least one income level for which this assumption holds.

¹¹ To save some notation the optimal private school quality choice of the corresponding household is always denoted by Q^* . It is obvious that the value of Q^* will change with income and ability.

implicit function theorem $\hat{y} = \hat{y}(\alpha; v, q, t, c_1, c_2)$ is a continuously differentiable function and the cutoff income level (for each ability) is decreasing in v and increasing in q, t, c_1, c_2 . Thus vouchers lead to both stratification by ability and income.

An interesting point to note is that the driving force behind sorting here is different from that in Epple and Romano (1998). In Epple and Romano, sorting arises due to an interplay of both demand and supply side factors. Here, in keeping with the feature of the Milwaukee experiment, private schools are not allowed to discriminate between students. Rather, they are modeled as passive, so that the supply side private school selection factor is absent. Sorting here arises exclusively due to the demand side factor of parental self-selection,—high income households at each ability level and high ability households at each income level self-select themselves to apply to the choice schools.

The underlying assumption so far has been that vouchers need to be topped up. Now consider the Milwaukee-type voucher system, where vouchers constitute the full payment of tuition. The above analysis reveals that random private school selection by itself cannot eliminate sorting,—I now examine whether random private selection coupled with absence of topping up of vouchers can do so.

Proposition 1 *Under $c_2 = 0$, there is sorting by ability, but no sorting by income in a Milwaukee-type voucher system (of no topping up).*

All proofs are in appendix A. The intuitive argument here is as follows. Vouchers enhance the choice options of public school households, but movement to private school is associated with a non-monetary cost c_2 . At each income level, only the higher ability households prefer to switch to a private school. This is because the higher ability households have a higher marginal valuation of school quality and hence are more willing to bear the switching cost. This leads to sorting by ability. The crucial driving force behind sorting by income is that households with different income are affected differently—while the higher income households find it profitable to pay private school tuition, or to top up the voucher amount, the low income households do not. When vouchers serve as the full payment of tuition and $c_2 = 0$, no monetary cost has to be incurred to switch to a private school. Therefore, at each ability

level, public school households of all income levels behave symmetrically and there is no sorting by income.

Corollary 1 *If $c_2 \neq 0$, vouchers lead to sorting by income in addition to sorting by ability.*

Under $c_2 \neq 0$, sorting by ability occurs for the same reason as above. Moreover, in the presence of a monetary relocation cost, there will be sorting by income. This is because at each ability level, only the higher income households find it profitable to pay this cost and switch to a private school. However, if c_2 is small, sorting by income is small. Note that monetary cost is likely to be small in Milwaukee (at least in comparison to programs that allow topping up), more so because the MPS is required to provide for the transportation costs of the transferring student.

4 Data

This paper uses the Milwaukee Parental Choice Program public release data files [Witte and Thorn (1995)]. They contain descriptive data on individual students and schools, student scores for ITBS reading and math and extensive survey data on both MPS students and students who applied to the choice program for the period 1990 through 1994. The response rates for the surveys varied between 46% and 50%, except one survey where the response rate was 30%. Descriptive characteristics of students consist of personal characteristics (age, birth date, grade, present school, distances to past and present school) as well as demographic characteristics (sex, ethnicity, whether free/reduced price lunch eligible).

Among the survey data files, this study mostly uses the wave 1 and MPS control group survey files. Wave 1 surveys were mailed in the fall of each year from 1990-1994 to all parents who applied for enrollment slots for their children in one of the choice schools for the first time in that year. MPS control group surveys were sent in March of 1991 to a random sample of 5,474 parents of students in Milwaukee Public Schools. The random selection was done by selecting children with birth dates that were the 15th or the 28th of any month. Among other purposes, the surveys were intended to assess parental knowledge of and evaluation of the choice program, educational experiences in prior public

schools, the extent of parental involvement in prior public schools, the expectations parents hold for their children etc.

5 Empirical Strategy

The empirical part of the paper seeks to test the theoretical predictions that in a Milwaukee-type scenario, there will be no (or very little) sorting by income, although there will still be sorting by ability. Household ability is measured by the following set of variables—mother’s education, the number of times the parent contacted the school in the prior year over various issues (“contact”), the number of times per week the parent participates in different activities with the child (“child-time”), whether the parents participated in parent-teacher organization and activities in the prior year (“PTO participation”), educational expectations of the parents and prior test scores of the child. Mother’s education, contact, child-time, PTO participation and educational expectations are respectively measured on scales of 0-2, 0-3, 0-3, 0-5 and 0-3. Higher values indicate higher levels of the corresponding variable.¹² The income measure used here is household income. It is measured in a scale of 0-3. In addition to considering mother’s education and household income as variables ranging on a scale of 0-2 and 0-3 respectively, I also run alternative specifications where I consider dummy variables for different levels of mother’s education and household income respectively. In such cases mother’s education is modeled by three dummies denoting categories: “Mother<High school graduate”, “Mother High school graduate” and “Mother>High School Graduate” and income by four dummies denoting categories: “Low income”, “Middle income”, “Upper-middle income” and “High income”. (For a more detailed description of these variables, see table B.2.)

Since the testable predictions relate to the case where the private schools pick students randomly, I first investigate whether this has been the case in practice. To investigate this, I first compare the demographic characteristics, household incomes and ability indicators of choice applicants accepted by

¹² For a more detailed description of the survey instruments, categories and codes, see appendix tables B.1 and B.2.

private schools (“accepted applicants”) with those of choice applicants who failed to get a private school seat (“non-accepted applicants”). Next using the dichotomous variable “accept” (that takes on a value of 1 for successful applicants and 0 for unsuccessful applicants), I estimate a multivariate logit model. The purpose is to investigate whether the probability of success depends on a systematic way on any of the underlying demographic variables, income or ability measures. Random selection would obviate any such relationship.

If random selection is supported by the data, I proceed to test for stratification by ability and income. For this purpose, I first compare the household income and different measures of ability of the choice applicants with that of MPS students who were eligible to apply but chose not to apply. Any MPS student who is eligible for free or reduced price lunch is eligible to apply to the MPCP.¹³ Therefore, using the random MPS survey, I extract the group of students who were eligible for free or reduced price lunches but did not apply. The group of students who applied to the choice program during 1990-94 form my first group.

Next I estimate a series of logistic regressions to test for sorting. I define a dichotomous variable “apply” that takes on a value of 1 for choice applicants and a value of 0 for eligible non-applicants. Stratification by income and ability would respectively dictate a positive relationship between the probability of “apply” and household income and a positive relationship between probability of “apply” and the different measures of ability.¹⁴ In each of these regressions, I control for student specific demographic and socio-economic factors. I use two alternative comparisons to check the robustness of the results. First, pooling together the choice applicants for all the years 1990-94, I compare their characteristics

¹³ Under the MPCP, all students with household income at or below 175% of the poverty line are eligible to apply for vouchers. Households at or below 185% of the poverty line are eligible for reduced price lunches and those at or below 135% of the poverty line are eligible for free lunches. The cutoff of 175% is not strictly enforced and households within this 10% margin are often allowed to apply (Hoxby (2003)). Moreover, almost 90% of the students who were eligible for free or reduced price lunches also qualified for the free lunch program (Witte (1997)). So there were very few students with household income between 175% and 185% of the poverty line.

¹⁴ To be more precise, stratification by income implies that at each ability level, students with higher income apply, while stratification by ability implies that at each income level, students with higher ability apply. Empirically, the coefficient of relevance for the former is the logit coefficient of income in a regression that controls for ability and other student-specific demographic variables. The coefficient of relevance for the latter is the coefficient of ability in a logit regression that controls for household income and other demographic variables.

with those of the eligible non-applicants. Second, I do a year-by-year analysis, where I compare the characteristics of the choice applicants in each year separately with the characteristics of the eligible non-applicant group.

A disadvantage of the above analysis is that it compares applicants and non-applicants across all schools, so that the effect of parental actions is confounded with that of school-specific actions. To get around this problem, I include school dummies, so that applicants from a certain school are compared to eligible non-applicants from that school only,—this enables me to get rid of any school specific effects (example, enthusiastic administrators encouraging movement and vice-versa). Witte (2000) and Witte and Thorn (1997) compares applicants with non-applicants and do not include school indicator variables.

A note on prior scores of students is in order here. First, a large number of applicants and non-applicants do not have data on prior scores. A reason for this is that the MPS tests students in their second, fifth, seventh and tenth grades, so that there are fewer prior test scores for students who enter the program in the lower grades (Witte and Thorn, 1997). Since there is heavy enrollment in the MPCP in the pre-kindergarten through second grade, a large proportion of the students do not have data on their prior scores. Still another problem is that the students who have data available on the prior scores may not be random samples of the applicant and non-applicant pools respectively. Therefore the initial logistic regressions used to investigate stratification in this study do not include prior test scores. (Results with prior test scores are reported in table 7.)

Second, the prior test scores of applicants (non-applicants) relate to the last *available* test before application (before survey), and in many cases this is not the last year's test. The available prior test scores relate to years 1984-94 while the data relates to applicants for the years 1990 through 1994. Third, the test scores relate to the different grades in which the ITBS was given. The analyses in Witte (2000) and Witte and Thorn (1997) pool the data on the prior scores of applicants and non-applicants, irrespective of the year and grade of test. In addition to doing this, I also include year, school and grade dummies that control for any idiosyncratic behavior relating to any particular year, school or grade.

Another difference with Witte (2000) and Witte and Thorn (1997) is in the use of the ability measures. They aggregate the different measures of parental involvement into a single measure of parental involvement or participation. I, on the other hand, allow the different measures of parental involvement (contact, child-time, PTO participation) to have different effects. Since the use of multiple measures might lead to multicollinearity, I also test for this problem, as discussed in the results section.

6 Results

The results are arranged in the following order in this section. Subsection 6.1 analyzes whether private schools picked choice applicants randomly as was required by the program. Subsection 6.2 investigates whether vouchers in Milwaukee led to stratification by ability and income.

6.1 Did Private Schools Pick Applicants Randomly?

Table 1 compares the summary characteristics of the accepted and non-accepted choice applicants in the MPCP during 1990-94. The two groups are very similar and there is no statistically significant difference between the two groups in terms of any of the demographic characteristics, income and ability measures.¹⁵ I also do a year by year comparison of the two groups in terms of each of these variables for the years 1990 through 1994. These results are very similar (and hence are not reported here), —once again there is no statistically significant difference between the two groups.

To investigate whether results from this bivariate analysis continue to hold when the different variables are considered simultaneously, I run a series of logistic regressions. Table 2 reports these results. The dependent variable is a dichotomous variable “accept” that takes on a value of 1 if the student was accepted by an MPCP private school and 0 if the student was unsuccessful in getting a private school seat under MPCP. Since the coefficients from a logit regression are relatively non-intuitive, I also report the corresponding odds-ratios. The first specification (columns (1)-(2)) includes gender, race, mother’s

¹⁵ In addition to comparing the two groups in terms of the overall contact, PTO participation and child-time variables, I also compare them on the basis of each of the component measures that constitute the overall measure. The results remain very similar and hence are not reported here. The component measures are outlined in appendix table B.1.

education and household income as regressors. The second specification (columns (3)-(4)) adds a more elaborate set of ability measures and a dummy indicating whether the mother was employed full-time. The third specification (columns (5) and (6)) reports results from a model that, instead of using the scaled variables mother's education and income, includes dummies for different levels of mother's education and household income.¹⁶ The final specification (columns (7)-(8)) includes prior reading and math normal curve equivalent scores as ability measures in addition to mother's education.¹⁷ The results are very similar across all the specifications—none of the variables are statistically significant, indicating that change in none of the variables has a significant effect on the probability of acceptance. There is robust evidence in favor of random selection—not only are the results from the logistic regressions consistent with those from the bivariate analysis, but inclusion or exclusion of variables do not change the coefficients or odds-ratios by much. Also the model chi-square is never significant, indicating that the null hypothesis that the coefficients are zero cannot be rejected. This further vindicates random selection.

6.2 Is there Stratification by Income and Ability?

Having established that random selection took place during the period under consideration, this section investigates whether sorting took place under random selection. Table 3 compares the summary characteristics of the choice applicants during 1990-94 with that of the eligible non-applicants. Mother's education of the choice applicants is considerably higher than that of the non-applicants and the difference is statistically significant. Consistent with this, the proportion of mothers in the lower levels of education is much lower and the proportion in the higher levels much higher for the applicants and these differences are statistically significant. The picture is similar for the other ability measures. In terms of the number of times the parents contact the child's school on a variety of issues, time spent with

¹⁶ Inclusion of dummies for different categories is useful in that this allows the different categories to affect the probability of application differently. The problem with this formulation, however, is that the presence of too many categorical variables may lead to a degrees of freedom problem.

¹⁷ Inclusion of the other ability measures in this regression leads to a drastic fall in the number of observations, so I do not include them here,—the results however remain similar.

the child in different activities (like reading, math, writing, sports etc.), educational expectations for the child, proportion of parents participating in various parent-teacher activities, the applicants score considerably higher than the non-applicants and these differences are always statistically significant. Applicant households are however, virtually indistinguishable from the non-applicant households in terms of household income and the very small differences between the two groups are never significant. Year-by-year comparisons of the different ability and income measures have very similar results and hence are not reported here.¹⁸ Thus the results obtained from the bivariate analysis are very much consistent with the theoretical predictions.

These patterns are mirrored in the graphical analysis (figures 1 and 2). While there is not much difference between the income distribution of the applicants and non-applicants, the distribution of the different ability measures are very different between these two groups. For each of the ability measures, the proportions of choice households in the upper categories are much higher than the corresponding non-applicant proportions while the proportions of applicants in the lower categories are much smaller. This again supports stratification by ability.

I now investigate whether the pattern that arises from the bivariate comparisons and the income-ability distribution figures is confirmed in a multivariate logistic regression framework. Using data from MPCP applicants for 1990-94 and eligible non-applicants from the MPS, table 5 reports the results from four alternative logit specifications. Columns (1)-(4) pool all the years together, while (5)-(9) do a year-by-year analysis. The dependent variable here is a dummy variable “apply” that takes on a value of one for choice applicants and a value of zero for eligible non-applicants. Each of the columns controls for race and sex of the student, columns (3), (4), (5)-(9) also control for an indicator variable indicating whether mother is employed full-time. All columns of this table report odds-ratios for easier interpretation, the corresponding logit coefficients are available on request. Odds ratio and standard error of the odds ratio are given by e^b and $S.E(b)e^b$ respectively, where b denotes the logit coefficient.

¹⁸ The only difference is that in 1994 the applicant households had economically and statistically higher income than the non-applicants.

Statistical significance corresponds to the logit coefficients, because it is the coefficient that is normally distributed, not the odds-ratio.¹⁹ The odds-ratio says by how much the odds of “apply” increases as the explanatory variable is increased by one unit.

The first column includes household income and the most basic ability measure, mother’s education. Column (2) includes a more elaborate set of ability measures. These include the number of times the household contacted the child’s school in the previous year (“contact”), whether the parents participated in parent-teacher activities (“PTO participation”), the time the parent spends with the child in various activities (“child-time”) in a normal week. Column (3) includes educational expectations of the parent for the child and for a dummy variable indicating whether the mother is employed full-time. The results are very similar across the different specifications. Household income is never statistically significant and its odds-ratio is always very close to one, indicating that income does not have much effect on the probability of application. On the other hand, all the ability measures are statistically significant (except child-time), with odds-ratios exceeding one by large margins. Consider the logistic regression in column (3). Although child-time is not significant, with the odds-ratio exactly equaling unity, the other three ability measures are highly significant. A unit increase in mother’s education increases the odds of application by 67%. A unit increase in the variables contact, PTO participation and child-time increases the odds of application by 7%, 12% and 62% respectively.

Using the specification corresponding to column (3), column (3') displays the impact on probability of application if each of the categorical income and ability measures are changed from their minimum to maximum values, while holding every other variable constant at its mean. Having a mother with a college degree rather than one who dropped out of high school increases the probability of application by 0.21. Having a household income of above \$20,000 rather than a household income of below \$5,000

¹⁹ Odds-ratio gives the ratio of odds of a one unit change in the explanatory variable and always takes the value e^b , where b denotes the logit coefficient, irrespective of the point of measurement. If p denotes probability, then odds at x is given by $\frac{p(x)}{1-p(x)}$, odds at $(x+1)$ by $\frac{p(x+1)}{1-p(x+1)}$ and the odds-ratio at x by $\frac{\frac{p(x+1)}{1-p(x+1)}}{\frac{p(x)}{1-p(x)}} = \frac{p(x+1)}{1-p(x+1)} \cdot \frac{1-p(x)}{p(x)}$. Using the logistic distribution, it can be easily seen that the odds ratio equals e^b , where b is the coefficient.

increases the probability of application by 0.04, however note that income is not statistically significant. Moving from the lowest category of the contact variable (parents never contacted the school) to the highest category (parents contacted school 35 or more times in the course of the previous year) increases the probability of application by 0.31. Students with parents who participated in all 5 parent-teacher activities considered have a 0.12 higher probability of applying compared to students whose parents never participated in any such activity. Parents who expect their students to go to graduate school have a 0.26 higher probability of sending their child to a choice school than parents who expect their child to just finish some high school. Spending time with the child, on the other hand, do not seem to have a major effect on the probability of application, which is perhaps not what one would have expected. Column (4) reports odds ratios from a logit regression that instead of using the previous formulation of mother's education and household income, includes dummies for various categories of mother's education and household income. The results are very similar to those obtained above.

Since considering all the five years together may camouflage some distinctive patterns that may be present in one or more years, I next do a year-by-year analysis. Household income is never statistically significant except in 1994 when the odds ratio equals 1.63. In each of the years, mother's education is highly significant, with the odds-ratios ranging from 1.75 in 1992 to 2.58 in 1994. The results for contact, PTO participation, child-time and educational expectations are also similar to those obtained from the earlier analysis.

Since the regressions above have multiple measures of ability, a potential concern here is multicollinearity. Using several methods, I find that multicollinearity is not likely to be a problem. First, the correlation between the different variables never exceed 0.4 and almost always lie between -0.1 and 0.2. Second, the estimates are very robust to dropping of variables. Finally, the variance inflation factor corresponding to the different coefficients never exceed 1.8. A rule of thumb often used is that a variance inflation factor above 10 indicates multicollinearity.

To rule out the effect of any school specific factors, I next run logistic regressions that control for

school dummies. These regressions compare choice applicants to eligible non-applicants within the same school. Columns (1) and (2) of table 5 pool all the years together while columns (3)-(7) report results for individual years. All regressions control for race and sex of the student. Column (1) includes mother's education and household income as regressors, while column (2) includes a more elaborate set of ability measures. The regressions for the individual years do not include the wider set of ability measures since this leads to a considerable fall in sample size, (although the results remain similar). Once again, table 5 shows that an increase in each of the ability measures (except child-time) increases the probability of application, in fact the effect for mother's education is now even stronger than before. The pattern in income is very similar to that obtained earlier.

Table 6 looks at the effect of prior reading and math scores of students on the probability of application. These logistic regressions also include mother's education, income, sex and other demographic variables. Since the prior scores pertain to different years, schools and grades, column (2) includes year dummies, column (3) year and school dummies and column (4) year, school and grade dummies. The patterns for mother's education and income are very similar to earlier, although household income is no longer significant in 1994. Prior reading score is never statistically significant. Although prior math score is initially significant, it no longer remains significant after inclusion of year, school dummies (column 3) and year, school, grade dummies (column 4). Moreover, the odds ratios for both prior reading and prior math scores are very close to one. This indicates that not only statistically, but economically also, an increase in prior scores has no effect on the probability of application. Prior scores are often looked upon as ability measures in the literature. However, it should be noted here that the above pattern does not suggest an absence of sorting by ability. While interpreting these results, it should be kept in mind that many students do not have data on prior scores (for example, just the inclusion of prior reading and math scores leads to a fall in sample size from 1638 (table 4, column 1) to 765 (table 6, column 1), and this smaller sample may not be a random sample of the applicants and eligible non-applicants.

The findings obtained in the empirical part of the paper can be summarized as follows. There is strong and robust evidence in favor of stratification by ability. Not only is it manifest in simple bivariate and graphical analyzes, but is also supported in a logistic framework. This pattern is robust to inclusion of school dummies and holds for a variety of ability measures such as mother's education, contact, PTO participation and educational expectations. There is not much evidence in favor of stratification by income. It is statistically significant only in 1994, even that effect no longer exists after inclusion of year-of-test, school, grade dummies and prior scores. (However, as discussed above, the regressions in table 6 should be interpreted with caution.) Moreover, the odds ratios for income are never very far from one (except 1994), which further provides evidence that income is not a major factor in the application decision. Thus the empirical findings strongly support the predictions obtained from theory.

7 Conclusions

This paper analyzes the impact of voucher design on student sorting and more specifically investigates whether random private school selection and the absence of topping up of vouchers can eliminate student sorting. Much of the existing literature investigates the issue of sorting where private schools can choose their students. However, in the publicly funded voucher experiments in the U.S.—Milwaukee, Cleveland, Florida and Washington DC—the private schools are not permitted, by law, to discriminate between students. This study addresses the issue of sorting in such circumstances, where the supply side force to sorting (private school screening) is absent. It focuses on the first five years of the Milwaukee voucher program.

The Milwaukee voucher program is characterized by both random private school selection and the absence of topping up of vouchers. In the context of an equilibrium model of household behavior, the paper argues that random private school selection alone cannot preclude sorting by income and ability. However, random private school selection coupled with the absence of topping up can obviate sorting by income, but not sorting by ability.

Implementing a logit estimation strategy, using multiple measures of ability and student level data for 1990-94, the paper then shows that the testable predictions are validated empirically. There is robust evidence in favor of stratification by ability but no consistent evidence of sorting by income. These results are robust to alternative specifications and various measures of ability. The paper makes an important contribution to the literature on sorting—it points out, both theoretically and empirically, that random private school selection along with the absence of topping up of vouchers can preclude sorting by income.

The findings of the paper have important policy implications. They strongly suggest that adequate design of voucher policy can prevent sorting by income. While random private school selection alone cannot obviate sorting by income, a voucher policy that combines both random private school selection and full payment of tuition by vouchers can preclude sorting by income.

Finally, it should be noted that the empirical findings of the paper pertain to the first five years of the Milwaukee program. As outlined earlier, following a Wisconsin Supreme Court ruling of 1998, religious schools were allowed to participate in the program for the first time in school year 1998-99. As a result, the Milwaukee voucher program saw a major expansion both in terms of the number of choice students and in terms of the number of choice schools. Unlike in the pre-1998 period, the majority of the schools in the latter period were religious. The predictions of the theoretical model continue to hold in the second phase (that is, after the shift) also as the key features that drive the results continue to be satisfied in the second phase of the program. Note that the participation of religious schools are likely to attract families of the corresponding religious affiliations more,²⁰ but within the various groups the predictions of the model should continue to hold, that is, there will be no (or very little) sorting by income, although there is likely to be sorting by ability. However, since adequate student level data are not available after 1994, there is no way to test their validity in this period.

²⁰ In the context of a national means tested voucher program, Campbell, West and Peterson (2005) show that Catholics and Evangelical Protestants were more likely to apply and take up vouchers, consistent with the fact that most of the choice schools were affiliated with the Catholic church or associated with Evangelical Protestant traditions.

Appendix A: Proofs of Results

Proof of Proposition 1. Before the imposition of vouchers, a public school household that is indifferent to switching to a private school satisfies the following equality:

$h(y - tQ^*) - c_1 + \alpha u(Q^*) = h(y) + \alpha u(q)$. For each income level and given other parameters, there is a unique $\hat{\alpha}_0$ that solves this equation:

$$\hat{\alpha}_0 = \frac{h(y) - h(y - tQ^*) + c_1}{u(Q^*) - u(q)} \quad \text{A.1}$$

After imposition of vouchers, a household that is indifferent between public and private options, satisfies $h(y + v - tQ^*) - c_1 + \alpha u(Q^*) = h(y) + \alpha u(q) \Rightarrow \alpha u(\bar{Q}) - c_1 = \alpha u(q)$, since vouchers are not allowed to be topped up. Note that since private school quality is not costly here, $Q^* = \bar{Q}$, where \bar{Q} is the highest private school quality. Since $D = \alpha[u(\bar{Q}) - u(q)] - c_1$ is independent of income, there is no sorting by income. Given other parameters, a unique ability level $\hat{\alpha}_1$ solves this equation:

$$\hat{\alpha}_1 = \frac{c_1}{u(\bar{Q}) - u(q)} < \hat{\alpha}_0 \quad \text{A.2}$$

$\hat{\alpha}_1$ is less than $\hat{\alpha}_0$ as the numerator of A.2 is smaller and the denominator larger than that in A.1.

Thus vouchers increase sorting by ability even when vouchers fully pay for private school tuition. ■

Proof of corollary 1. *Sorting by ability:* Before the imposition of vouchers, a public school household that is indifferent to switching to a private school satisfies the following equality:

$h(y - tQ^* - c_2) - c_1 + \alpha u(Q^*) = h(y) + \alpha u(q)$. For each income level and given other parameters, $\hat{\alpha}_2$ solves this equation where $\hat{\alpha}_2$ is given by:

$$\hat{\alpha}_2 = \frac{h(y) - h(y - tQ^* - c_2) + c_1}{u(Q^*) - u(q)} \quad \text{A.3}$$

After imposition of vouchers, a household that is indifferent between public and private options, satisfies $h(y - c_2) - c_1 + \alpha u(Q^*) = h(y) + \alpha u(q)$. Given other parameters, a unique ability level $\hat{\alpha}_3$ solves this equation:

$$\hat{\alpha}_3 = \frac{h(y) - h(y - c_2) + c_1}{u(\bar{Q}) - u(q)} < \hat{\alpha}_2 \quad \text{A.4}$$

$\hat{\alpha}_3$ is less than $\hat{\alpha}_2$ as the numerator of A.4 is smaller and the denominator larger than that in A.3.

Thus vouchers increase sorting by ability.

Sorting by income: Before the imposition of vouchers, for each ability level and given other parameters, there exists a unique income level \hat{y}_0 that satisfies the following

$$h(y - tQ^* - c_2) - c_1 + \alpha u(Q^*) = h(y) + \alpha u(q) \quad \text{A.5}$$

After the imposition of vouchers, for each ability level and given other parameters, there exists a unique income level \hat{y}_1 that solves the equation:

$$h(y - c_2) - c_1 + \alpha u(\bar{Q}) = h(y) + \alpha u(q) \quad \text{A.6}$$

Note the left hand sides of A.5 and A.6 are identical. Since $\bar{Q} > Q^*$ and $(y - c_2) > (y - tQ^* - c_2)$, it follows that $\hat{y}_1 < \hat{y}_0$. This implies that vouchers increase sorting by income. ■

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**Table 1: Summary Characteristics of Accepted and Non-Accepted Choice Applicants,
MPCP (1990-94)**

	Accepted (std. dev.) (1)	Non-Accepted (std. dev.) (2)	Accepted–Non-Accepted [p-value] (3)
Proportion Male	0.45 (0.50)	0.50 (0.50)	-0.06 [0.12]
Proportion African American	0.78 (0.41)	0.79 (0.41)	-0.01 [0.81]
Proportion Hispanic	0.15 (0.36)	0.14 (0.35)	0.01 [0.69]
Proportion White	0.05 (0.23)	0.05 (0.23)	0.00 (0.99)
Proportion Asian	0.00 (0.04)	0.00 (0.06)	0.00 [0.56]
Mother's Education	1.40 (0.74)	1.39 (0.76)	0.01 [0.82]
Household Income	1.47 (0.95)	1.52 (1.00)	-0.05 [0.11]
Proportion of mothers employed full-time	0.40 (0.49)	0.39 (0.49)	0.01 [0.76]
Contact	8.73 (5.02)	9.07 (4.69)	-0.34 [0.35]
PTO Participation	2.42 (1.50)	2.39 (1.41)	0.03 [0.76]
Child-time	10.63 (4.15)	10.75 (4.19)	-0.13 [0.72]
Educational Expectations	2.31 (0.70)	2.33 (0.72)	-0.02 [0.72]
Prior Reading NCE Score*	40.18 (17.56)	38.90 (15.72)	1.28 [0.49]
Prior Math NCE Score*	40.25 (19.60)	37.25 (20.19)	2.99 [0.19]
Number of Students	750	346	

Household income, mother's education, contact, PTO participation, child-time and educational expectations are scaled variables. Higher values indicate higher levels of the corresponding variable. For a detailed description of the variables, refer to tables B.1 and B.2. *Many students do not have data on prior test scores so that the sample size falls to 255 (230) for the "accepted" and 123 (111) for the "non-accepted" in reading (math).

Table 2: Did Private Schools Pick Students Randomly?
Comparing Accepted and Non-Accepted Applicants, MPCP 1990-94
(Using Logistic Regressions)

	Odds-Ratio (1)	Coeff. (2)	Odds-Ratio (3)	Coeff. (4)	Odds-Ratio (5)	Coeff. (6)	Odds-Ratio (7)	Coeff. (8)
African American	0.99 (0.32)	-0.01 (0.32)	1.08 (0.52)	0.07 (0.48)	1.13 (0.55)	0.13 (0.48)	0.80 (0.56)	-0.22 (0.70)
Hispanic	1.00 (0.37)	0.00 (0.37)	1.86 (1.09)	0.62 (0.58)	1.93 (1.14)	0.66 (0.59)	0.43 (0.32)	-0.85 (0.75)
Mother's Education	1.10 (0.11)	0.10 (0.10)	1.12 (0.20)	0.12 (0.18)			1.09 (0.18)	0.08 (0.16)
Household Income	0.87 (0.07)	-0.13 (0.09)	0.89 (0.12)	-0.12 (0.13)			0.82 (0.10)	-0.19 (0.13)
Contact			0.99 (0.03)	-0.01 (0.03)	0.98 (0.03)		-0.02 (0.03)	
PTO Participation			0.91 (0.08)	-0.09 (0.09)	0.92 (0.08)		-0.08 (0.09)	
Child-time			1.04 (0.03)	0.04 (0.03)	1.04 (0.03)		0.04 (0.03)	
Educational Expectations			0.84 (0.16)	-0.18 (0.19)	0.83 (0.16)		-0.19 (0.19)	
Mother Employed full-time			1.37 (0.35)	0.32 (0.25)	1.48 (0.40)		0.39 (0.27)	
Mother High School Grad					1.05 (0.42)		0.05 (0.40)	
Mother> High School Grad					1.16 (0.46)		0.15 (0.39)	
Middle income					1.32 (0.53)		0.28 (0.40)	
Upper-Middle income					1.06 (0.42)		0.06 (0.39)	
High income					0.79 (0.33)		-0.23 (0.42)	
Prior Reading NCE score							1.00 (0.01)	0.00 (0.01)
Prior Math NCE Score							1.01 (0.01)	0.01 (0.01)
Observations	852	852	293	293	293	293	314	314
Probability > Chi-Square	0.45	0.45	0.68	0.68	0.80	0.80	0.51	0.51

* , **, ***: significant at the 10, 5, and 1 percent level, respectively. Huber-White standard errors are in parentheses. The even numbered columns report the coefficients, while the odd numbered columns report the odds-ratios. Household Income, Mother's Education, Contact, PTO Participation, Child-time and Educational Expectations are scaled variables. Higher values indicate higher levels of the corresponding variable. Mother High School Graduate, Mother> High School Graduate, Middle-income, Upper-Middleincome, High-income are dummy variables for the corresponding level of mother's education and income respectively. For a detailed description of the variables, refer to tables B.1 and B.2.

Table 3: Summary Characteristics of Choice Applicants and Eligible Nonapplicants, MPCP (1990-94)

	Appl. (s. d.) (1)	Eligible Non-Appl. (s.d.) (2)	(1)-(2) [p-value] (3)		Appl. (s. d.) (4)	Eligible Non-Appl. (s.d.) (5)	(4)-(5) [p-value] (6)
Mother's Education	1.40 (0.74)	0.92 (0.81)	0.48 [0.00]	PTO & PTC (Proportions):			
Proportions in groups:				Attend PTC	0.91 (0.29)	0.81 (0.39)	0.10 [0.00]
Less than High	0.16 (0.36)	0.37 (0.48)	-0.21 [0.00]	Belong to PTO	0.22 (0.41)	0.15 (0.36)	0.07 [0.00]
High School Graduate	0.29 (0.46)	0.34 (0.47)	-0.05 [0.02]	Attend PTO Meetings	0.55 (0.50)	0.36 (0.48)	0.19 [0.00]
More than High	0.55 (0.50)	0.29 (0.45)	0.26 [0.00]	Attend PTO Activities	0.50 (0.50)	0.29 (0.45)	0.21 [0.00]
Household Income	1.44 (0.96)	1.42 (1.00)	0.02 [0.66]	Child-time:			
Proportions in groups:				Read/Week	2.05 (0.90)	1.67 (1.08)	0.38 [0.00]
< \$5,000	0.17 (0.38)	0.20 (0.40)	-0.03 [0.11]	Math/Week	1.85 (1.00)	1.68 (1.09)	0.16 [0.00]
\$5,000-\$9,999	0.37 (0.48)	0.34 (0.47)	0.03 [0.22]	Writing/Week	1.83 (1.01)	1.42 (1.13)	0.41 [0.00]
\$10,000-\$19,999	0.29 (0.45)	0.28 (0.45)	0.01 [0.56]	T.V/Week	1.63 (0.96)	1.44 (1.08)	0.19 [0.00]
>\$20,000	0.17 (0.37)	0.17 (0.38)	0.00 [0.54]	Sports/Week	1.35 (1.01)	1.27 (1.11)	0.09 [0.08]
Contact:							
About Academic Performance	1.82 (1.06)	1.33 (1.05)	0.48 [0.00]	Proportion of Mothers	0.40	0.33	0.07
About Classes Child took	1.22 (1.09)	0.85 (0.93)	0.37 [0.00]	Employed Full-time	(0.49)	(0.47)	[0.00]
About Volunteer Work	1.04 (1.09)	0.49 (0.88)	0.55 [0.00]	Educational Expectations	2.32 (0.70)	1.98 (0.82)	0.34 [0.00]
About Fund Raising	1.15 (1.00)	0.59 (0.82)	0.57 [0.00]				
About School Records	1.21 (0.93)	1.01 (0.92)	0.20 [0.00]				
About Child's Behavior	1.51 (1.19)	1.22 (1.10)	0.28 [0.00]				
About Helping in Class	0.92 (1.06)	0.50 (0.89)	0.42 [0.00]				

Household Income, Mother's Education and the various contact and child-time variables are scaled variables. Higher values indicate higher levels of the corresponding variable. For a detailed description of the variables, refer to tables B.1 and B.2.

Table 4: Is there Stratification by Ability and Income?
Comparing Choice Applicants and Eligible Non-Applicants, MPCP 1990-94
(Using Logistic Regressions)

	1990-94					1990	1991	1992	1993	1994
	(1)	(2)	(3)	(3')	(4)	(5)	(6)	(7)	(8)	(9)
Mother's Education	2.07*** (0.15)	1.77*** (0.18)	1.67*** (0.19)	0.21		1.88*** (0.28)	2.02*** (0.29)	1.75*** (0.20)	1.98*** (0.37)	2.58*** (0.62)
Household Income	0.95 (0.05)	1.04 (0.08)	1.06 (0.10)	0.04		1.09 (0.13)	0.90 (0.10)	0.97 (0.12)	0.93 (0.14)	1.63*** (0.31)
Contact		1.07*** (0.02)	1.07*** (0.02)	0.31	1.07*** (0.02)	1.06*** (0.02)	1.06** (0.03)	1.10*** (0.03)	1.03 (0.03)	1.08* (0.05)
PTO Participation		1.16** (0.07)	1.12* (0.07)	0.12	1.12* (0.07)	1.22*** (0.09)	1.24*** (0.10)	1.06*** (0.09)	1.21* (0.12)	1.00 (0.12)
Child-time		1.00 (0.02)	1.00 (0.02)	-0.01	1.00 (0.02)			1.00 (0.02)	0.96 (0.03)	1.03 (0.04)
Educational Expectations			1.62*** (0.18)	0.26	1.61*** (0.17)	1.10 (0.14)	1.36** (0.19)	1.55*** (0.21)	1.76*** (0.34)	1.81*** (0.40)
Mother High School Graduate						1.48* (0.34)				
Mother > High School Graduate						2.72*** (0.63)				
Middle income						1.00 (0.24)				
Upper-Middle income						1.16 (0.30)				
High income						1.15 (0.34)				
Observations	1638	1013	905			905	737	723	713	657
Probability > Chi-Square	0.00	0.00	0.00			0.00	0.00	0.00	0.00	0.00

*, **, ***: significant at the 10, 5, and 1 percent level, respectively. This table reports odds-ratios. Huber-White standard errors are in parentheses. Statistical significance corresponds to the logit coefficients, because it is the coefficient that is normally distributed, not the odds-ratio. Odds ratio is given by e^b , where b denotes the logit coefficient. All regressions control for race and sex of student. Columns (3), (4), (5)-(9) control for an indicator variable denoting whether mother is employed full-time. Household Income, Mother's Education, Contact, PTO Participation, Child-time and Educational Expectations are scaled variables. Higher values indicate higher levels of the corresponding variable. Mother High School Graduate, Mother > High School Graduate, Middle-income, Upper-Middleincome, High-income are dummy variables for the corresponding level of mother's education and income respectively. For a detailed description of the variables, refer to tables B.1 and B.2.

Table 5: Examining Stratification by Comparing Choice Applicants and Eligible Non-Applicants Within Schools

	1990-94		1990	1991	1992	1993	1994
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mother's Education	2.09*** (0.20)	1.84*** (0.29)	2.70*** (0.58)	1.86*** (0.31)	2.21*** (0.41)	2.42*** (0.48)	2.84*** (0.90)
Household Income	1.03 (0.08)	1.04 (0.11)	0.94 (0.11)	0.87 (0.11)	1.11 (0.15)	0.93 (0.12)	1.69*** (0.38)
Contact		1.08*** (0.03)					
PTO Participation		1.19** (0.10)					
Child-time		0.96 (0.03)					
Educational Expectations		1.83*** (0.26)					
School dummies	Y	Y	Y	Y	Y	Y	Y
Observations	1250	741	550	537	540	415	300

Table 6: Comparing Prior Test Scores of Choice Applicants and Non-Applicants, 1990-94

	(1)	(2)	(3)	(4)
Mother's Education	1.68*** (0.18)	1.59*** (0.24)	2.73*** (0.70)	2.73*** (0.73)
Household Income	0.96 (0.08)	1.00 (0.11)	0.89 (0.14)	0.91 (0.16)
Prior Reading NCE Score	1.01 (0.01)	1.01 (0.01)	1.01 (0.01)	1.01 (0.01)
Prior Math NCE Score	0.99*** (0.01)	0.99*** (0.01)	0.99 (0.01)	0.99 (0.01)
Year dummies	N	Y	Y	Y
School dummies	N	N	Y	Y
Grade dummies	N	N	N	Y
Observations	765	585	352	346

Notes for tables 6 and 7: *, **, ***: significant at the 10, 5, and 1 percent level, respectively. This table reports odds-ratios. Huber-White standard errors are in parentheses. Statistical significance corresponds to the logit coefficients, because it is the coefficient that is normally distributed, not the odds-ratio. Odds ratio is given by e^b , where b denotes the logit coefficient. All regressions control for race and sex of student. Household Income, Mother's Education, Contact, PTO Participation, Child-time and Educational Expectations are scaled variables. Higher values indicate higher levels of the corresponding variable. For a detailed description of the variables, refer to tables B.1 and B.2.

Table B.1: Survey Questions and Codes Corresponding to Some of the Data Variables

Survey Question	Survey Categories	Codes
Contact:		
During your child's last year in school, how many times (or someone in your household) contact the school about each of the following?		
Your Child's Academic Performance	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
The Classes Your Child Took	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
Doing Volunteer Work at School	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
Participating in Fund Raising	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
Providing Information for School Records	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
Your child's behavior	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
Helping in classroom	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
PTO Participation:		
During you and your spouse/partner do any of the following at your child's public school last year?		
Attend parent teacher conferences	Yes/No	1/0
Belong to a parent/teacher organization	Yes/No	1/0
Attend meetings of parent teacher organization	Yes/No	1/0
Take part in activities of parent teacher organizations	Yes/No	1/0
Belong to other organizations dealing with school matters	Yes/No	1/0
Child-time:		
How many times in a normal week would you say you participate in the following activities with your child?		
Read with or to your child	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
Work on arithmetic or math	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
Work on penmanship or writing	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
Watch educational programs on TV with your child	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
Participate together in sports activities	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively
Other homework	0, 1-2, 3-4, 5 or more	0,1,2,3 respectively

The responses corresponding to each question were grouped in the survey categories shown. These categories were correspondingly designated by the above codes. The regressions include overall measures of contact, PTO participation and child-time which are obtained by combining the respective component measures using an additive scale. "Contact" ranges from 0-21, "PTO participation" from 0-5 and "child-time" from 0-18.

**Table B.2: Survey Questions and Codes Corresponding to Some of the Data Variables
(Continued)**

Survey Question	Survey Categories	Codes
Educational Expectations:		
How far do you expect your child to go in school? (Educ. Expec.)		
Finish some high school	1	0
Graduate from high school	2	1
Go to vocational school after high school	3	1
Go to College	4	2
Go to graduate or professional school after college	5	3
Mother's Education		
Please check the highest education level for female parent/guardian		
Eighth grade or below	1	0
Some high school	2	0
GED	3	1
High School Graduate	4	1
Some college	5	2
4-year college	6	2
Post-graduate work	7	2
Household income:		
What is your family/household income range for one year?		
\$0-2,999	1	0
\$3,000-4,999	2	0
\$5,000-7,4999	3	1
\$7,500-9,999	4	1
\$10,000-14,999	5	2
\$15,000-19,999	6	2
\$20,000-24,999	7	3
\$25,000-34,999	8	3
\$35,000-49,999	9	3
\$50,000 or more	10	3

The responses corresponding to each question were grouped in the survey categories shown. These categories were correspondingly designated by the above codes. In addition to considering mother's education and household income as variables ranging on a scale of 0-2, I consider dummy variables for different levels of mother's education and household income respectively. In such cases mother's education is modeled by three dummies denoting categories: "Mother<High school graduate", "Mother High school graduate" and "Mother>High School Graduate" and income by four dummies denoting categories: "Low income" (\$0-4,999), "Middle income" (\$5,000-9,999), "Upper-middle income" (\$10,000-19,999) and "High income"

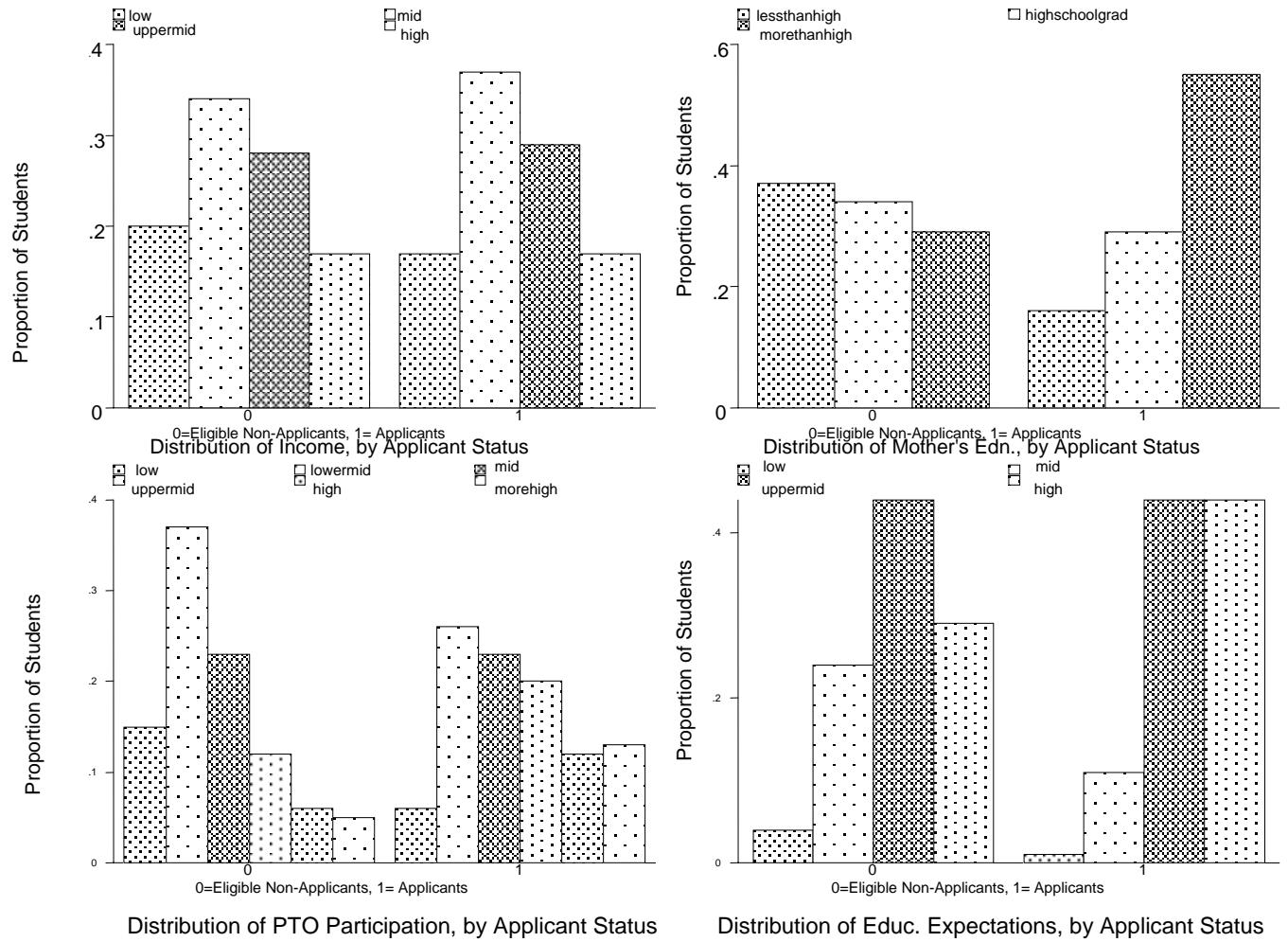
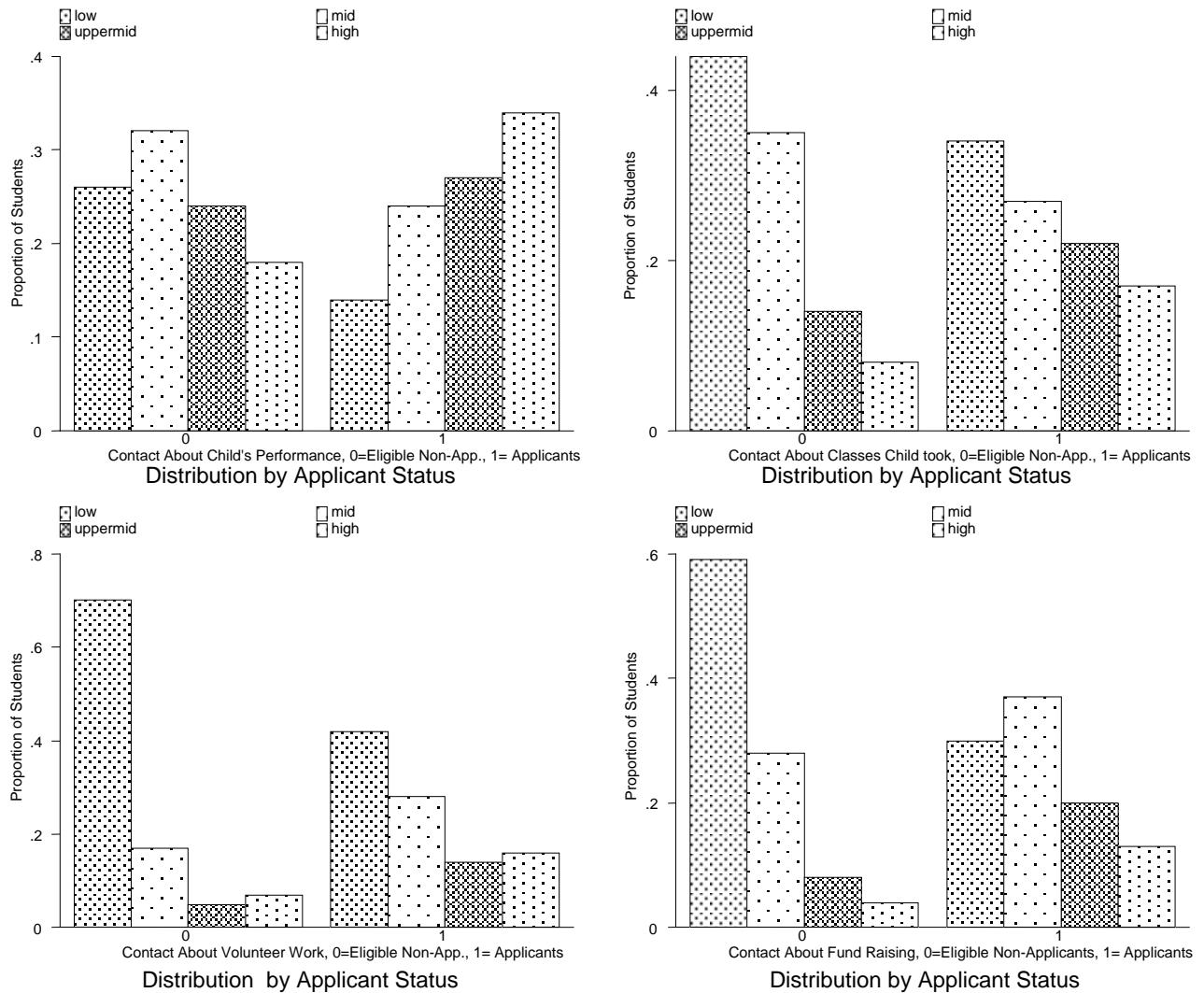
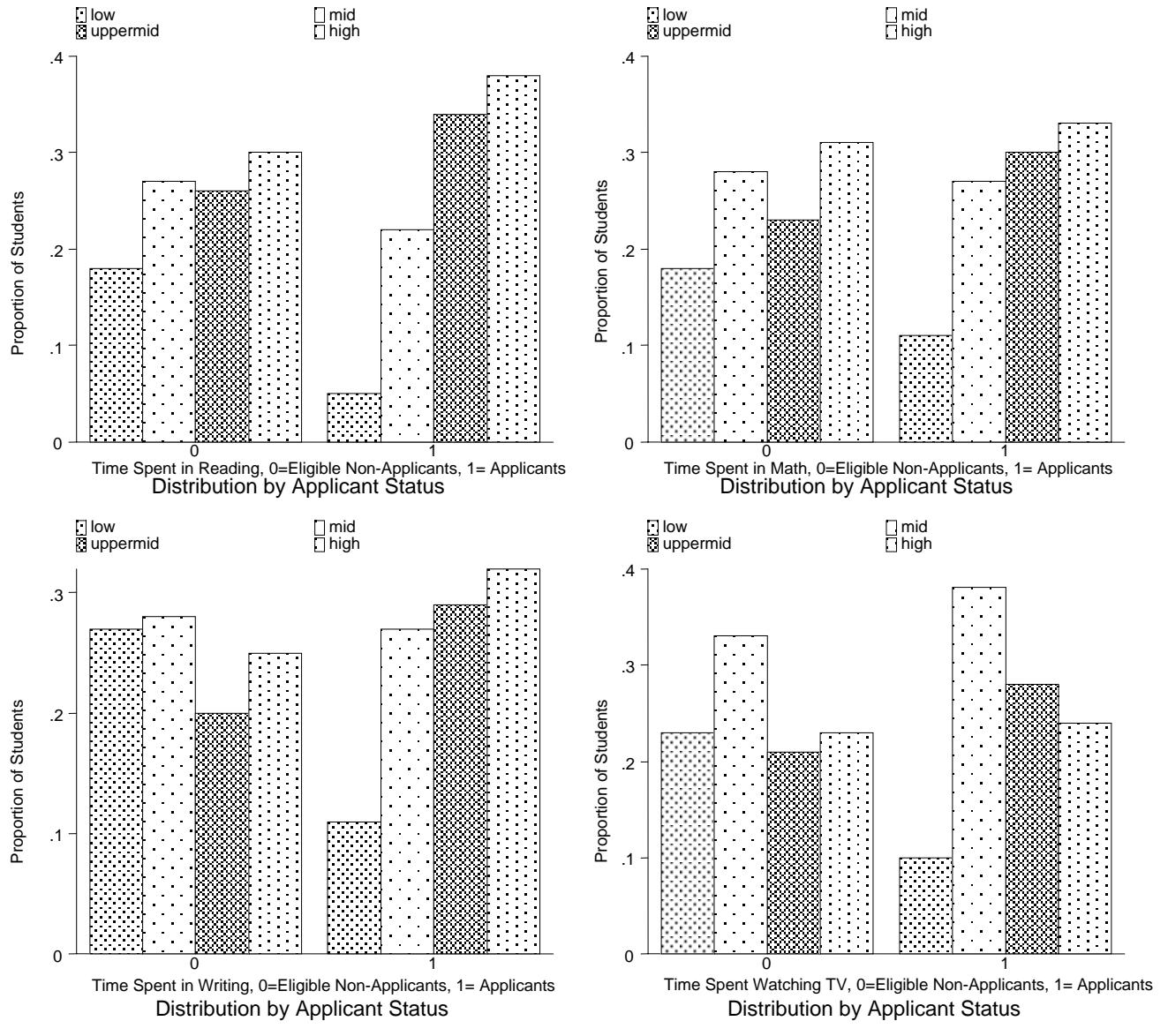


Figure 1. Stratification by Income and Ability, MPCP (1990-94)



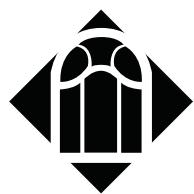
Ability Measure: Parents Contacting Child's School

Figure 2. Stratification by Ability, MPCP (1990-94)



Ability Measure: Time Spent by Parent With Child

Figure 3. Stratification by Ability, MPCP (1990-94) (Continued)



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